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# SPREAD OF CHOLERA

BY

PERSONAL COMMUNICATION,

AS

SEEN IN THE CRIMEAN CAMPAIGN.

BY

A. W. P. PINKERTON, M.D.,  
F.R.C.P.E.

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## THE SPREAD OF CHOLERA.

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SIX theories have been brought forward as to the main causes of the rise and progress of cholera. These are fully stated in the Report of the College of Physicians of London.<sup>1</sup> Dr Martin says, "That (theory) alone is supported by a large amount of evidence which regards the cause of cholera as a matter increasing by some process, whether chemical or organic, in impure and damp air, and assumes that, although, of course, diffused with the air, it is also distributed and diffused by means of human intercourse."<sup>2</sup>

From all we have seen in the Crimea, and from what has been published, we think this conclusion of Dr Martin the most correct. Within a *certain* limit, the disease may be carried in streams or currents of air, and may develop itself in a susceptible being without actual personal communication; but to any *considerable* distance, persons or material of some sort, not the atmosphere, are the main agents. And thus we think that the following propositions may be laid down as proved:—

1. Cholera spreads to *any considerable distance*—beyond 500 yards—by personal communication, not by the atmosphere.

2. When once the "Cholera poison" is carried to a spot favourable for its development, it will there attach itself, lying dormant

<sup>1</sup> 1. "General Atmospheric Influence," or "Epidemic Constitution." 2. Contagion: Poison undergoing increase *only* in the body. 3. Poison applied to mucous membranes, chiefly by water. 4. Poison reproduced in atmosphere and spread by *air*. 5. Poison increased by fermentation, etc., in impure air, and spread by human intercourse. 6. The material causes of the disease may be increased and propagated in and by impure air, as well as in and by the human body."

<sup>2</sup> Dr Martin on *Tropical Climates*, p. 313.

for a period of time, not yet fixed by any observations—certainly not less than four months, as the following observations show,—and is always most likely to break out among strangers living over the spot, in a house, hut, or ship.

3. Men so living, fall ill during a time of fatigue, watchfulness, and irregularity of living, as in trench duty, or during an epidemic of diarrhœa.

I. It is distinctly stated, and considered as a fact, by the French reporters, that cholera spread from Marseilles and Toulon to Gallipoli, Baltzick, and Varna. We are informed by them, that this disease appeared among the troops on board the French transports *before* they reached Malta,—*i. e.*, two or three days *after* they had sailed from the infected ports of Marseilles and Toulon.<sup>1</sup> By landing and embarking invalids at Gallipoli, Varna, and Baltzick, they spread the disease on shore, as well as on board other vessels. The “Primauguet” from Toulon, an infected vessel from an infected port, anchored 300 to 400 yards to the windward of H. M. S. “Diamond,” at Baltzick, July 14, 1854. A heavy breeze blew over the unhealthy vessel directly to the “Diamond,”—in fact, this French vessel lay *in shore* of all the English squadron. On the 16th July, a true case of cholera occurred on board “the Diamond,” forming the *first instance* among the English fleet in the Black Sea.<sup>2</sup> The “Diamond” had, like all other vessels, suffered from diarrhœa; but it was not till *after* the arrival of the infected French vessel, and not till after a direct current of air had swept from the one to the other, that the cholera showed itself.<sup>3</sup> This disease was unknown in the Black Sea that year, until the French troops came. And that this influence did not spread very far beyond the 400 yards, is proved by the fact, that the “Diamond” recovered her health by standing out to sea a short distance. At Scutari, where our army lay by themselves *before* they went to Varna, cholera was unknown, until the disease had spread among the troops in Bulgaria and the Crimea, and the invalids and convalescents had themselves been sent there. Again, in Malta, where *no French troops landed*, we had *no cholera*. M. Baudens says, that a direct band “tied together” Marseilles and Varna.<sup>4</sup> And Dr Mackay, one of our best naval medical observers, writes thus,—“I obtained sufficient information both from others, and my own personal observa-

<sup>1</sup> See *Med. Times and Gazette*, Dec. 1857, and *Annales d'Hygiène*, Janvier 1857.

<sup>2</sup> *Admiralty Report on the Cholera which attacked the Fleet in the Black Sea*, August 1854, etc.

<sup>3</sup> This way of accounting for the “Diamond” being infected is expressed with some hesitation, because if one could only learn the *true* previous history of each man on board, some more direct channel might be found. And it may yet turn out, as it has in many instances already, some one man, or substance, had been in cholera-stricken places.

<sup>4</sup> *Annales d'Hygiène*, Janvier 1857.



tion, to satisfy me, that the epidemic was an importation from France—the result of personal communication.”<sup>1</sup>

The French troops carried the disease with them to the Dobrudscha, where, M. Baudens asserts, cholera did not prevail previously.<sup>2</sup> And they suffered very severely, I presume, from changes and vicissitudes of a campaign throwing the constitution so much more open to attacks of disease.

A sudden outbreak at Baltzick among the fleets was immediately *after* General Bosquet's division had come from the interior (where it had suffered from cholera), and had encamped over the town, and communicated with the vessels. A French writer, M. Senard, tries to prove that these outbreaks were due to the atmosphere.<sup>3</sup> But if so, why did not all the vessels suffer alike and at the same time? Why did cases occur here and there independently of all others? Why did two vessels, lying close alongside one another, suffer in very different proportions? We are told by Dr Smart, a naval surgeon of great experience, that though “the cholera had become *extinct* in the ships of war, it was introduced into *some of the transports among the troops* ;”<sup>4</sup> in other words, the vessels which suffered were those carrying the men who had previously been subjected to cholera poison, and not the whole fleet, although they all breathed a common atmosphere. Again, we are told by Dr Mackay, that in H.M.S. “*Agamemnon*,” the cases were confined to the sailors who were employed for some hours or days on board the transports, not among the crew who had remained on board continuously.<sup>5</sup>

It was on the 21st and 22d July 1854, that the first cases occurred among our troops at Varna (five or six days after the “*Diamond's*” case), and after French soldiers had died in Varna Hospital. And immediately, owing to too favourable circumstances, the disease rapidly spread, and Varna became an infected port.

The second division of our army lay for two months at Scutari, where they enjoyed good health.

On the 20th June, the 47th Regiment landed at Varna, along with the 49th, 41st, 55th, 30th, and 95th. They had no cholera among them at the time of landing, although diarrhoea was more or less always prevalent. They lay for some time (two weeks?) two miles away from Varna, and then marched to Yanksecova, some twelve miles inland, during the first week of July. After being encamped for a fortnight or three weeks, and having communication with Varna, cholera broke out among the 30th and 55th Regiments. About these I am not so well informed; but I am sure, from the account of an eye-witness, Dr W. Whyte, that, in the 47th, cholera appeared on the 1st August, *i.e.*, seven or eight days *after* the disease

<sup>1</sup> Private letter to the writer.

<sup>2</sup> See *Medical Gazette*, Dec. 1857. p. 610.

<sup>3</sup> *Annales d'Hygiène*. Janvier 1857.

<sup>4</sup> Appendix, No. 8, *Report of Sanitary Commission in Crimea*.

<sup>5</sup> Dr Mackay's “*Notes on the Cholera in the Black Sea.*”

appeared generally in Varna, and, be it remarked, *after* a draft had joined the regiment from Varna. When this regiment first landed at Varna, cholera was unknown to exist anywhere; but after a draft of men had arrived from the port, which had, in *the meantime, become infected*, the disease appeared.

The 72d Highlanders left Malta, May 22d, 1855, healthy. The men were crowded on board a fine, new, large screw steamer, the "Alma," on her first trip. At Balaclava, some drafts for other regiments were disembarked, and the 63d Regiment, 300 strong, sent on board. The "Alma" then steamed away for Kertch, having the 72d, a healthy body of soldiers, and the 63d, already under cholera influence, on board; for this regiment had been decimated by disease, and had lately suffered from cholera in the front. We cast anchor below Kertch, half a mile from shore, and far away from the fleet. The first day, some fifteen men, an officer, and myself, were sent on shore for water. Next morning two cases of genuine cholera appeared, not among the men who had been ashore. After this, the cases so increased in number, that at last it was deemed necessary to move to another anchorage. This was done on the morning of the sixth day after our arrival. The same night we steamed back with despatches to Balaclava (the Anapa expedition being abandoned), and we shook off the disease. Now, why did the 72d, a healthy body of men from a healthy port, suffer on board a vessel which had never been at sea before? Some would say, this was a case of a body of strangers coming into an atmosphere saturated with cholera poison,—bad water, a scorching sun, and over-crowding, being the exciting causes. The party on shore might be cited as the conductors of the disease from the Crimea to the vessel. The true explanation, however, I think, was the presence of the 63rd Regiment on board. They brought the disease with them from the front, and they infected the 72d. There was no other visible cause. The party that went on shore did not suffer more than the rest; one company did not lose more men than another—all suffered equally. The water, over-crowding, food, heat, all were similarly submitted to, by the two different bodies of men; the 63rd having only one case, not fatal.

The 72d landed, encamped for one night outside Balaclava, and then, June 13th, marched to the front. Here they remained free from cholera until the 17th night of June, when two cases appeared, two days after the 31st Regiment had camped closed alongside, bringing the disease with them from Balaclava. The coincidence of the 31st coming with cholera close to the 72d one day, and the 72d suffering two days *after* from the same disease, is curious, and may be explained by personal communication. But we rather incline to the belief, that the disease was latent among the 72d, and developed by trench-work on the night of the 17th June, independent of the 31st. For it was in the trenches, among men who had been there for the first time, that the disease appeared, lasting



for three weeks, and causing thirty deaths. The 72d shifted their camp three weeks after all cholera had ceased, the end of July, to a new dry piece of ground behind the Guards. In August, when all were healthy, a draft, 200 strong, arrived from England. Within three weeks fifty of these were dead, or invalided, from most severe attacks of cholera. The draft had slept in the old tents, among the men previously subjected to cholera influences, and they suffered. At the same time, a draft of equal strength for the 71st joined the 72d, and lived separately in their own tents. They only lost two men; and the disease in them was remarkably mild and chronic. With the exception of being isolated, the 71st draft was every way similarly situated as the 72d draft; and yet the disease was not developed to any extent among them.

So late as November 1855, cases occurred among new-comers, and confined to them alone, who lived and mixed with the soldiers already cholera-stricken, even although huts, camping ground, everything were changed.

These facts, then, warrant us, we conceive, in saying, that men brought the disease, and that they were the main agents of its diffusion, altogether independent of the atmosphere. And, although cases like that of the "Diamond" may occur within *certain* limits, such atmospheric agency is circumscribed and easily avoided. To satisfy myself on this point, I wrote to Dr Mackay, asking him if he had facts to show that a healthy vessel, with a healthy crew, would suffer in an infected port, *without* any communication with persons or boats from shore. He gives the instance of the "Diamond," already quoted, as an example of the poison being conveyed a *short* distance, and so spreading. "But," he adds, "although I imagine that the poison of which we are speaking can be conveyed a *certain* distance through the atmosphere in *streams* or *strata*, I am not inclined to believe that it is so *diffused* in the atmosphere, that the whole of the air in that locality is rendered pestilential."<sup>1</sup> And that, to any considerable distance, the poison is conveyed by men, or bodies of men, not by the organic constituents of the atmosphere, is, I think, proved by the facts hastily brought forward. It is not to be argued that, unless the French brought the disease, our army would have escaped cholera. The malady is making itself endemic in Europe; and our troops were only too liable to every form of disease in Bulgaria. But the order of the appearance of the disease among the French, and then amongst us—its following the troops and abiding in the regiments till the cold of winter checked it, so like the course it pursued during the fifteen years it took to reach Great Britain—its following steadily, and at the *same pace*, the track of ships and commerce,—all tend to the conclusion, that the poison is conveyed by ourselves.

2. And when it is thus brought to any spot favourable for its

<sup>1</sup> Private Letter.

development, it will there attach itself, lying dormant for a period of time not yet defined, and always most likely to break out among strangers. The 79th Highlanders encamped at the foot of the Marine Heights near Balaclava, during the autumn of 1854, bringing cholera with them. In December, the disease gave place to diarrhœa, and this again to typhus. Damp, low-lying huts were the apparent chosen haunts of disease. In April 1855, the regiment was removed. On the 25th May, the 31st, a newly arrived corps, were put into the huts abandoned by the 79th. Between the 1st and 16th June, they lost 34 men from cholera, which began six days *after* coming to the infected spots. The companies that suffered most were those in the lowest, dampest huts; and when they were removed higher up to a drier position, they suffered still, though much less severely. On the 15th June, the 31st were marched up to the ground next the 72d in front, and there they lost 16 more men. On the 8th September 1855, 500 artillery were disembarked at Balaclava, and sent into the huts left empty by the 31st, two months previously. The men put into the damp, unhealthy huts, suffered from cholera, while those put in the drier ones did not present a single case. And when the unhealthy huts were pulled down and re-erected higher up, on dry soil, only one case showed itself, in all probability the result of the previous unhealthy position.<sup>1</sup>

Here we have a localised instance of cholera affecting *only* the persons subjected to the immediate influences of huts built on a damp spot, to which cholera had been brought the foregoing autumn. The 79th had carried the disease with them; it had occurred in these huts, and had been deposited in the soil, and there it lay latent from December, until the heat of a June sun developed it among the 31st. After they went away, it again lay dormant, until the susceptible and newly arrived artillerymen came under its influence. And that it was only in certain spots, not in the huts themselves, is proved by the men in the other huts escaping, and by the fact, that when these very same huts were pulled down and re-built over new, dry, clean soil, no cholera occurred, save the one case already mentioned, and which, doubtless, had gained an existence in its victim before leaving the old spot.

The 82d Regiment landed in September, and encamped on an old camping ground of the guards, and they suffered severely. The 92d Highlanders, at the same time, landed and encamped at Kamara, on a clean, dry piece of ground never before occupied, and they lost only six men.<sup>2</sup> And although, at this late date, it were impossible to make exact statements, we would merely say, that our army suffered at Devnagh very severely, while it was encamped over the ground occupied by the Russian troops in 1829-30, when they were decimated by cholera. And if one required another cause for this

<sup>1</sup> Dr Sutherland's *Report (Sanitary Commission) of Army in Crimea.*

<sup>2</sup> *Report of Sanitary Commission.*



scourge in Bulgaria, in addition to the one formerly mentioned as existing at Varna, the marshy soil and the latent poison might be adduced. In these instances, one could not blame the water, bad food, or mode of life, as the exclusive causes, because, while all suffered these last alike, few comparatively were attacked by cholera; and these few always under certain circumstances, either in tainted spots, or near infected persons or those who had already undergone the choleraic ordeal. Diarrhœa, scurvy, enteric typhus, dysentery, were all produced directly by bad food, and prostration of mind and body; that is, they were produced *in the body*. Cholera, again, appeared independent of those causes, and seemed capricious in its choice of victims.<sup>1</sup>

In the instance of the artillery, the whole body were newly arrived, they ate the same food, drank the same water, were exposed to the same duties, every way identical, save in the one company being in healthy huts on dry ground, and the other in unhealthy huts on a damp soil, where cholera had previously existed; and even when these huts were taken down and re-erected, they did not perpetuate the disease. This local cause is the same as was witnessed in 1837 on board the hospital ship "Dreadnought," moored off Greenwich. Here the cholera broke out in a man who had come from Dantzic, an infected port, eight days after he had been on board for a scalp wound; cases followed in other men from all parts of the world, on board this same ship; but there was no spreading of the disease either to other ships or to shore.<sup>2</sup> The first man here came from an infected port, and he remained twelve days on board the "Dreadnought," which had suffered from two distinct cholera outbreaks previously, before the disease developed itself in him. The poison lay dormant on board somewhere, and in some shape, since 1832-3, until it was brought into action by this man; the two, the vessel and the man, acting as passive agents, just as perhaps did take place between our troops and the tainted ground in Bulgaria. And that such spots do exist, we know from Indian records, where distinct lines can be traced between the healthy and unhealthy positions.

The "Dreadnought" offers an example of a man subjected to the air of what may be called a tainted locality, viz., the hold, during cleansing operations, being seized with cholera. The poison there lay latent until called into action by the first patient, and by being stirred up subsequently. And further, the man attacked, out of seven employed in cleansing out the foul hold, was the one who superintended the operations, but did not work. The other six,

<sup>1</sup> "I believe only because, like other contagious poisons, its effects on individuals were generally, *in part*, a result of *concurrent and accessory causes*,—sometimes in modes hitherto obscure, augmenting the *tendency* to it in the living body,—sometimes augmenting its own specific virulence, perhaps even developing its own nature."—*Dr Alison*.

<sup>2</sup> *Medico-Chirurgical Transactions* (London), vol. xxi.

similarly exposed, were saved by their constant working, and by their not standing idle like the superintendent. And this explains also, I think, why medical men and nurses are so often saved while attending sick,—the mental and physical occupation being the true safeguard.

3. Men exposed to the poison in such localities, suffered during a time of fatigue, watchfulness, irregularity of living, and often during an epidemic of diarrhœa.

In the Crimea, trench duty excited the disease,<sup>1</sup> as witnessed in the two first cases in the army, May 1855, and by myself in the 72d Regiment, and in the draft. It cannot be positively asserted that the poison did not lurk in the trenches themselves, as well as in the floors of huts or tents; but it is more consistent with our knowledge of accessory and concurrent causes to say, that the night work developed the previously-contracted disease. Very often, after a shower, when the men were wet, perhaps chilled, and the ground exhaling moisture, the greatest number of attacks were observed, showing the developing agency of the cold and wet, as well as adding another argument in favour of the depositing of poison in certain localities. The men were very liable to disease from the difference of life, and the total change of habits; while ardent spirits (which, though absolutely necessary in small quantity), taken on an empty stomach, or in excess, only paved the way for cholera. Again, depression of spirits, such as succeeded the failure of the 18th June 1855, told severely on the health of the troops. The men suffered much from diarrhœa; and, in many instances, cholera supervened. But from all I saw in the East of the true, rapidly fatal Asiatic cholera, I could not find out any connection between the two. They appeared distinct diseases; and though, perhaps, diarrhœa was very likely to develop cholera in a weakened constitution, there was no absolute relation. Cholera is a disease of the sympathetic system, a shock given to those nervous centres, totally independent of any affection of the intestinal mucous membranes.<sup>2</sup>

In every camp where cholera prevailed constantly, *clay* soils were found. The Report of the Sanitary Commission plainly shows that the light dry soils were always the healthy ones, and that all the bad unhealthy stations were on clayey ground, and, in many instances, green fungous growths in great luxuriance were found underneath the planking of the huts. This tenacious clay, the “Crimean mud,” was of a dense consistency, and would effectually prevent absorption of fluid downwards. It was different, too, from the marshy plains of the Tchernaya, which were so fruitful in causing intermittents among our allies, though very few miles separated each seat of disease.

Such are the facts which were observed in the East. They dis-

<sup>1</sup> *Report of Sanitary Commissioners.*

<sup>2</sup> See Vol. v., *Med. Review* for 1838, Bellingin (of Turin) on Cholera, p. 235; and Dr Fr. Chaumont, *Edin. Journal* for 1854.



tinently show that the French brought the disease from Marseilles and Toulon, and that, intermingling with our fleet and army, they imparted the disease to us. By personal communication, contact of healthy with sick is not necessary. A ship, tent, a man, or a man's clothes, may be the conductors of the disease from one victim to another. To produce cholera, the *germs* or *poison* of the disease must first be carried to the spot by *ourselves*. This agrees with what has been stated, and with the slow progress of the epidemic during its first appearance in Asia and Europe, after intervals of quite uncertain duration. And when once it becomes established in a place, or among a community, it may break out again and again under certain developing circumstances,—the nature of which are as much beyond our knowledge, as the reasons why small-pox, typhus, or measles will appear alternately, or be absent for years. When once the poison of cholera has taken effect on the body of a patient, we know more, than in the case of these other epidemics, of the concurrent and accessory causes which may come into operation, whether fermentation of the ejecta in privies, and in clay soils, or the propagation of the disease by insects or animalcules, as proposed by Sir H. Holland,—which theory certainly tends to unravel many of the more obscure phases of cholera, and which would satisfactorily explain those instances like the “Diamond,” where the air for a *short* distance was tainted with poison. We have no evidence of cholera occurring in a healthy ship, among a healthy crew, and on the high seas. In other words, we have no proof of the spontaneous origin of cholera in a previously healthy locality. The “New York” sailed from Havre, a healthy port, with emigrants, November 1848. Cholera appeared on board sixteen days after leaving port. But *the persons who suffered were those who had come from Hamburgh, and other parts of Germany, where the disease prevailed.*<sup>1</sup> And on board this vessel, as on board the “Apollo,” etc., heat developed the latent malady, just as heat and showers did it in the East.

Height above the sea-level had no marked effect on our troops, as many contracted the disease in one spot, and sickened at another. The most fruitful sites of disease were in the valleys, and on the sea coast at the mouth of the harbour of Balaclava, Kamiesch, etc.

In limiting the atmospheric agency, and in contenting ourselves with those causes over which we may have control, we obtain some advantage over this insidious foe. We know its locality, its habits, the kind of soil and of atmosphere favouring it; we know what may develop it; and we know what sometimes checks it. And it is important to observe, as to it—probably as to all morbid agents—stench or any odours need not accompany it. Frequently it makes its appearance where no stench or perceptible filth exists. These two are often combined, but not necessarily so. And, as is seen constantly, disease appears in places as clean as we can make them.

<sup>1</sup> *Report of Royal College of Physicians (London) on Cholera.*

We have experience showing us that standing out to sea a short distance, removing bodies of men to another encampment, even no farther than twenty or thirty yards, will check the disease. Dr Mackay says in his letter, "I am of opinion that where the poison does not meet with some organised being, it will soon become diluted, dissipated, and destroyed. For this reason thorough ventilation, segregation of the sick from the healthy, separation of the sick from each other, as far as practicable, will prove beneficial."<sup>1</sup>

An incomprehensible disease, such as cholera, will never be fully understood, so long as we are prone to physical maladies. And it would be useless, endless work, were we to attempt to unravel the ways and means whereby Providence changes the type of disease, or the type of human constitutions. Cholera, like other epidemic maladies, comes and goes, how, when, and where, to a great extent, we know not. And we have no right to know. To a certain degree, however, we can see; facts observed lead us to reason on them, and, as in the foregoing observations, we cannot avoid drawing conclusions. These are necessarily very limited; for we cannot change the constitution of ourselves, of disease, or of the atmosphere. We see, however, that man can carry the disease, and can communicate it; that, altogether independent of wind and weather, the disease may progress in a given direction. Moreover, we know that in certain localities it may hide itself; and, further, that to a great extent it may be avoided. Hence quarantine—which has been so held up to scorn, as an opprobrium on our profession, and a proof of our utter ignorance—if *judiciously performed*, would be of infinite service. But such results are hardly to be expected, so long as our utilitarian and money-making age exists. It is impossible to combine the most lucrative commerce with sanitary regulations. This is the reason why no justice has been done, as yet, to preventive measures as a branch of our profession. And until attention be paid to these, and personal interests be sunk in national precautions, we may in vain attempt to limit disease.

If we look on cholera as a poison, "increasing by some process, chemical or organic, in impure and damp air," and that it is carried by men from place to place, we have a certain amount of control over the disease. And, certainly, the means adopted in the Crimea, and the care taken of the men, were so successful and complete, that in April 1856, there were only five deaths on Russian ground, and twenty-two in the hospitals of Smyrna, Sentari, Renkioi, and Kertch, out of a force of 72,000 men!! That this, in part, was owing to the less severe duties, and less exposure, and to the suspension of hostilities, is true; but if preventive measures had not been taken previously, disease would have found too many idle victims; and had it not been for the cleansing of Balaclava and the harbour, that port would have continued a hotbed of pestilence.

<sup>1</sup> Private Letter.



One other remark in conclusion. We cannot limit the period of incubation of the cholera poison. Men were attacked three days to fourteen after contact, or after first coming under vitiated influences. The 92d lay six days in harbour before they disembarked, and they suffered after marching to Kamara. They got the poison among them while they lay on board; and it was developed by the march and living in canvas—such a life being new to the men. The 72d draft suffered within the first ten days or fortnight. And how long the poison may lie latent in a soil, ship, house, or even clothes, is not to be hastily limited. A curious form of fever broke out on board the “Rosamand” (the yellow-fever-stricken “Eclair”), in the Black Sea, among the men employed cleaning out her hold. It was unlike all the other fevers seen in the fleet, and it was confined to the men who were subjected to the air from the holds which had long lain foul. Was this not a modified form of “yellow fever,” even though ten years had elapsed since the vessel had suffered?

In treating of such diseases as cholera and fever, we must not attempt to limit time, cause, or effect. All we can do is, to prevent disease by avoiding what we see plainly to be *one* of the causes. And, if we do so, we will accomplish more by well-timed precaution, than we will by active treatment after the disease has appeared amongst us.

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## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXXVII., 1857-58. MEETING VII.

*Professor Alison* said a few words in commendation of the zeal and industry, and likewise of the intelligence and judgment, of one of the youngest members of the Society, who had laid before them the results of pretty extensive observation on the extension of cholera. When he spoke of intelligence and judgment, it might be attributed to vanity, that he begged the Society to observe that the conclusions to which Dr P. had come were exactly the same as had impressed themselves on the minds, he believed, of all who had seen much of the first epidemic in Scotland in 1832, and which he had himself, since that time, uniformly endeavoured to impress on the students in Edinburgh—particularly that it seemed now generally admitted, that the epidemic cholera originated from a specific poison, quite distinct from any of the natural causes of bowel complaints; and that its extension could not be explained without believing, *both* that this poison, under certain circumstances, might acquire intensity, attach itself to places, and cause an *endemic* disease after the manner of a *malaria*; and likewise that it might attach itself to, lie latent for a very variable time in, and be propagated and communicated, to a very various extent, by the living human body—so causing *epidemics*, the extension of which appeared to depend on conditions differing considerably from any other epidemic disease; although it had not been sufficiently observed, as he thought, in the discussions of this subject, particularly in London, that every contagious disease, and even every returning epidemic of such disease, had some peculiarities by which it might be distinguished from others. He thought Dr P.'s paper particularly important, because, particularly when taken in connection with some other, equally careful and practical observations, and equally re-

moved from all theoretical prejudices, it established the efficacy of some of these, local and temporary, accessory circumstances and conditions. He mentioned the observations of Dr Budd of Bristol, Dr Snow in London, and Dr Acland in Oxford, which could hardly be explained without supposing that *impure* (or rather *poisoned water*) was the medium of communication. The effect of damp argillaceous soils, of cold and wet, of mental depression, fatigue and defect of sleep, of the previous habits of body, and of the time of life most liable to acute disease, as exemplified in this paper, were all indicated likewise by various observers in Scotland as well as in the East; he could refer particularly to very similar facts, brought together by one of his earliest medical friends, Sir H. Holland, in the chapter in his *Medical Facts and Reflections*, where he treats of "the Hypothesis of Insect (or Animalcular) Life as a Cause of the Diffusion of Cholera;" and that, under certain circumstances not yet fully understood, and in some seasons, what is first a cause of malaria, tainting a district, may become a contagious poison, diffusing itself by the human body, he thought ascertained by another excellent friend and careful observer, Dr McWilliam, in his account of the endemic fever of Bon Vista, and the subsequent epidemics which H. M. ship "Rosamond" appeared to have undergone, under various names.

Professor Simpson said, the Society was much indebted to Dr Pinkerton for the excellent paper he had just read; and he was sure, that while the Society expressed its thanks to its youngest member, it had equally to express its great gratification that the subject should have been such as to call forth the remarks of its oldest and most esteemed member, Dr Alison. The same cause which had induced Dr Pinkerton to turn his attention to this subject, viz., the teaching and lectures of Dr Alison, had also led Professor Simpson to publish a collection of cases and observations regarding the contagious character of cholera some twenty years ago. Professor Simpson said he believed one great advance had been made; that some *modes* of communication of this disease were at the present day known to us, of which we were ignorant at that earlier date, and which would serve to explain much regarding its propagation which had then remained a mystery. The propagation and spread of cholera by contagion had undergone various changes since that time. When the second epidemic was approaching, many authorities had come forward against the contagious propagation of the disease; but after the disease did reach us, the previous belief in its power of contagious propagation became apparently more decided than ever. In illustration, Dr Simpson adduced two series of cases in which no mode of contagious propagation of the disease could at the time be discovered; but where later inquiries rendered it highly probable that the malady was begun to be spread through limited localities by the use of water which the dejections of persons previously affected had reached. Some modes in which the disease spread were still quite unintelligible upon our present principles and knowledge,—but a deeper study of them might be hoped to withdraw in some degree the mystery. It had been proved, that cholera had a far greater tendency to occur in low than in high localities; and the contagious character of the disease was sometimes proved by this law being defied. Professor Simpson mentioned an instance where a servant in Westmoreland had left her situation infected with this disease, and although her friends resided in one of the highest inhabited localities in England, she carried the disease home with her, and led to its outbreak in her native hamlet. There was one question he would like to ask, viz., Was there any instance known of cholera appearing on board a ship out at sea, and not from an infected port? The Americans generally, he understood, did not believe the disease to be contagious. But at the time cholera was introduced into that continent, he was not aware of any case occurring in vessels coming from America, while the disease was common in our ships going out to that country. During the time that a quarantine station was established at Staaten Island, the physicians and nurses who came down there from New York, which was then quite healthy, were im-



mediately seized with the disease. This seemed a good experiment to test the existence of contagion. Much might be done, he thought, to prevent the spread of this and other diseases by contagion. If the French fleet had been sent away from a healthy and not from an infected French port to the East, possibly the ravages of cholera in our Eastern navies and armies might have been entirely saved. He was struck by a remark of Dr Walshe's, as exemplifying this point, where there is mentioned the occurrence of a number of deaths by hooping-cough at St Helena, the contagion having been introduced with a quantity of clothes sent from a ship into the island to be washed. We all knew, he said, the curious hygienic law laid down of old by the Jews, ordaining that every one should go beyond the camp for the purpose of defecation, etc., and not only so, but carry a spade along with him to bury what was evacuated ; this was a true system of prevention. Professor Simpson concluded by stating, that by a little precaution the effects of contagion in cholera, as indeed in all diseases, might be obviously guarded against to a very great extent.





